Kafirs, that "an American Quaker, West, took the statue of the Belvedere Apollo for the representation of a Mohawk Indian." Having made Mr. Wood talk this extraordinary nonsense, he then reviles him for being illogical. This is not the treatment Mr. Wood merits. No one denies the faults of his work, especially the unhappy straining after the picturesque which has made so many of his artist's illustrations worse than worthless. But his genial and suggestive descriptions of South African native life give a permanent value to his popular volume, while in his special line as a student of savage arts and implements, Dr. Fritsch can hardly expect to rival him.

EDWARD B. TYLOR

## *LETTERS TO THE EDITOR*

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

### Flowers of the Primrose destroyed by Birds

FOR above twenty years I have observed every spring in my shrubberies and in the neighbouring woods, that a large number of the flowers of the primrose are cut off, and lie strewn on the ground close round the plants. So it is sometimes with the flowers of the cowslip and polyanthus, when they are borne on short stalks. This year the devastation has been greater than ever; and in a little wood not far from my house many hundred flowers have been destroyed, and some clumps have been completely denuded. For reasons presently to be given, I have no doubt that this is done by birds; and as I once saw some greenfinches flying away from some primroses, I suspect that this is the enemy. The object of the birds in thus cutting off the flowers long perplexed me. As we have little water hereabouts, I at one time thought that it was done in order to squeeze the juice out of the stalks; but I have since observed that they are as frequently cut during very rainy, as during dry weather. One of my sons then suggested that the object was to get the nectar of the flowers; and I have no doubt that this is the right explanation. On a hasty glance it appears as if the foot-stalk had been cut through; but on close inspection, it will invariably be found that the extreme base of the calyx and the young ovary are left attached to the foot-stalk. And if the cut-off ends of the flowers be examined, it will be seen that they do not fit the narrow cut-off ends of the calyx, which remains attached to the stalk. A piece of the calyx between one and two-tenths of an inch in length, has generally been cut clean away; and these little bits of the calyx can often be found on the ground; but sometimes they remain hanging by a few fibres to the upper part of the calyx of the detached flowers. Now no animal that I can think of, except a bird, could make two almost parallel clean cuts, transversely across the calyx of a flower. The part which is cut off contains within the narrow tube of the corolla the nectar; and the pressure of the bird's beak would force this out at both the cut-off ends. I have never heard of any bird in Europe feeding on nectar; though there are many that do so in the tropical parts of the New and Old Worlds, and which are believed to aid in the cross-fertilisation of the species. In such cases both the bird and the plant would profit. But with the primrose it is an unmitigated evil, and might well lead to its extermination; for in the wood above alluded to many hundred flowers have been destroyed this season, and cannot produce a single seed. My object in this communication to NATURE is to ask your correspondents in England and abroad to observe whether the primroses there suffer, and to state the result, whether negative or affirmative, adding whether primroses are abundant in each district. I cannot remember having formerly seen anything of the kind in the midland counties of England. If the habit of cutting off the flowers should prove, as seems probable, to be general, we must look at it as inherited or instinctive; for it is unlikely that each bird should have discovered during its individual life-time the exact spot where the nectar lies concealed within the tube of the corolla, and should have learnt to bite off the flowers so skilfully that a minute portion of the calyx is always left attached to the foot-stalk. If, on the other hand, the evil is confined to this part of Kent, it will be a curious case of a new habit or instinct arising in this primrose-decked land.

Down, Beckenham, Kent, April 18 CH. DARWIN

### Signor D'Albertis' and Dr Meyer's Discoveries in New Guinea

HAVING just returned to Europe, I read in NATURE, vol ix. p. 77, a communication which contains an assertion of Dr. A. B. Meyer, to the effect that I did not cross New Guinea at all, and that he claims the honour of having done so himself.

From what Dr. Meyer says, the public are led to believe that I have claimed the honour of crossing this unknown and little-explored island; if he had read "A Month among the Papuans of Mount Arfâk," he might easily have ascertained that I never asserted this. There the reader will see that I only claimed to have penetrated the country to a distance of thirty miles, and to have ascended to a height of between 3,000 and 4,000 feet; but I *was* the first European to see alive and shoot many rare Birds of Paradise peculiar to New Guinea. One of these was entirely new to science, and has been called *Drepanornis albertisi* by Dr. Sclater (NATURE, vol. viii. p. 305); it may be the same bird subsequently described as new by Dr. Meyer.

I have no wish to deprive the last-named gentleman of the honour of having crossed a greater or lesser portion of New Guinea, but I object most decidedly, either indirectly or by insinuation, to being deprived of the credit of being the first European to penetrate into the interior of that interesting country.

April 20 LUIGI MARIA D'ALBERTIS

### Spontaneous Generation

MR. RAY LANKESTER's letter in last week's NATURE affords fresh evidence of his lack of acquaintance with the several stages through which the "spontaneous generation" controversy has passed, or he would not now cite as a "most important result" only made known by recent experimentation, a fact which has been well known and repeatedly verified since the time of Spallanzani. I allude to the influence of the prolongation of the period of exposure to heat in retarding or altogether arresting the putrefactive tendencies of organic solutions. I have not thought it needful on previous occasions to point out the various misconceptions and the apparent ignorance of facts shown by Mr. Lankester in his querulous communications to your columns on the subject of "Spontaneous Generation." There are one or two points, however, to which I will now venture to solicit his attention, and that of your readers generally.

Mr. Lankester says:—"It is probably now familiar to those interested in the matter, that the experiments of Dr. Sanderson have established the fact that in an infusion of turnips and cheese prepared as directed by Dr. Bastian, heating to a temperature of 102° C. is sufficient to prevent the subsequent development of life (Bacteria) in the infusion, even when the exposure to that temperature is only maintained for a few minutes." To this statement I have to add that since the publication of the experiments above alluded to by Dr. Sanderson, I have heated flasks, sealed in the ordinary way and containing the fluid above mentioned, to a temperature of 105° C. for ten minutes in a chloride of calcium bath, and have found these fluids swarming with Bacteria after six days. I have also heated in the same manner simple neutralised turnip-infusion (filtered through cotton-wool instead of filtering paper) to a temperature of 105° C. for ten minutes, and by subsequently keeping these less putrescible fluids at a higher temperature (about 35° C.) they became turbid and swarmed with Bacteria in three days. Neither Dr. Sanderson's experiments nor those of Mr. Lankester and Dr. Pode have, therefore, the cogency which Mr. Lankester imagines them to possess. But, as I have endeavoured to point out on a previous occasion (NATURE, vol. viii. p. 548), experiments of this kind at the present stage

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of the controversy can teach us nothing definitely as to the death-point of Bacteria and their germs, though they are of interest with regard to the question of the degree of heat which suffices to check the productivity of the fluids in question.

We are now told that Mr. Lankester himself, and those with whom he sides, are agreed as to the fact that Bacteria are killed at "a temperature a little below 70° C." Of course I cannot tell to what extent Mr. Lankester is in possession of the views of Prof. Huxley and others, but if what he states is really true, the statement is of a reassuring nature; it looks like progress, and leads me to hope that the only remaining doubt may soon be solved. How long does it take for the "through-heating" of certain "possible" Bacteria germs? This is now the knotty problem which, according to Mr. Ray Lankester, seems alone to require solution before we can positively decide as to the heterogenetic origin of Bacteria. Perhaps I may help him on his way to the solution of this difficulty by calling his attention to certain experiments made in Calcutta by Dr. Timothy Lewis, in reference to the existence of living tape-worm germs in cooked meat ("Report of Sanitary Commissioners with the Government of India, 1871"). Dr. Lewis says:—"The temperature of legs of mutton which had been put into the boiler almost as soon as the water was put into it averaged 140° F. (60° C.) in the interior at the moment the water had reached the boiling point (212° F.), and after boiling for five minutes the temperature had reached 170° F. (76° C.)." Now with these facts in his possession, and with some suggestions from physicists of his acquaintance as to the mode of conduction of heat generally, Mr. Lankester may perhaps soon solve his problem, so far as this is practicable. The problem itself may be stated thus:—If the through-heating of several pounds of protoplasm in the shape of a leg of mutton, when immersed in water, takes place at such a rate as to raise the central portions of the joint to a temperature of 60° C. by the time the water has reached 100° C., and if the exposure of the leg of mutton to this heat for the space of five minutes suffices to raise its central portions from 60° to 76° C., how many seconds, minutes, or hours will it take to heat an infinitesimal part of a grain of protoplasm (all through) to the temperature of 76° C.—that is, to a degree of heat decidedly above the death-point of bacterial protoplasm as given by Mr. Ray Lankester? The Bacterium-germ in question, it must be recollected, cannot be supposed to have undergone any extreme amount of desiccation previous to its immersion in the experimental fluid, since such desiccation would have already destroyed its life, according to Dr. Sanderson.

Whilst Mr. Lankester is seeking the solution of the problem above stated, perhaps he might with advantage also reflect a little more closely upon the possible value or otherwise of some of the negative results to which he is so fond of alluding. It is perhaps scarcely necessary for me to remind Mr. Lankester that the obtaining of such negative results is always easy, and may show nothing more than the relative incapacity of the experimenter for performing careful work according to instructions. Not long ago Mr. Lankester, upon the strength of his own negative results, triumphantly announced that he was about to prove to the world the falsity of my views, and so help to justify the opinion which he at the same time expressed as to my being "the mesmerised victim of delusion," "an abnormal psychological phenomenon," and many other fine things. But unfortunately for Mr. Lankester, just about the same time Dr. Sanderson (whose opinions he so much respects) had an opportunity of satisfying himself that I could demonstrate the experimental results which Mr. Lankester failed to obtain. Dr. Sanderson helped to show, in fact, that my positive results were worth more than the many negative results obtained by other workers.

Finally, I think it necessary to add a few words concerning the views of my colleague, Dr. Sanderson, on the subject of heterogenesis, simply because I find his experiments and supposed views frequently quoted by Mr. Lankester, and others, as evidence of the erroneous nature of my conclusions.

I have been led by my experiments to believe in Heterogenesis and also in Archebiosis, but I regard the recognition of the present occurrence of Heterogenesis as of far more importance than the recognition of Archebiosis. Now the controversy between Needham and Spallanzani, and also that between Pasteur and Pouchet was as to the present occurrence or non-occurrence of heterogenesis. This was what they understood, and what the majority of people at the present day still understand, as "Spontaneous Generation." And as to the reality of this process, Dr. Sanderson has been convinced. He admits that Bacteria may appear in flasks, and other situations, where we are warranted in believing that no bacterial matter pre-existed—which is exactly equivalen to a belief in "Spontaneous Generation," in the sense implied by Pasteur and others. In support of this statement I have only to make the following quotations from his papers and reported speeches of the last two years. Referring to experiments made in 1871, Dr. Sanderson says: "Bacteria could not be shown to be present either actually or in germ in the healthy liquids or tissues, or in the products of healthy inflammation" (*British Medical Journal*, May 11, 1872, p. 508). This statement was made with reference to man, and also to the lower animals with which he had experimented. In another part of the same communication as it stands revised in the "Transactions of the Pathological Society," for 1872, Dr. Sanderson says: "If a few drops of previously boiled and cooled dilute solution of ammonia are injected underneath the skin of a guinea-pig, a diffuse inflammation is produced, the exudation liquid of which is found, after twenty-four hours, to be charged with Bacteria." Other chemical agents will act in the same way even when every precaution against external contamination has been adopted; and as a drop of this fluid introduced with equal care into the peritoneum of another animal is always capable of exciting the phenomena of pyæmia, Dr. Sanderson has made known the very important fact that this process "can be proved to be capable of originating from inflammations produced by chemical agents *under conditions which preclude the possibility of the introduction of any infecting matter from without.*" Again, in a speech delivered last month before the Clinical Society, and reported verbatim in the *British Medical Journal* for March 24, Dr. Sanderson insists upon the complete establishment of the truth of this latter proposition both for man and the lower animals. He says: "We must admit that the whole process of pyæmia can originate in the organism independently of external influences." But, as he also says: "In every pyæmic inflammation—whether it be a primary or a secondary one—in every form of pyæmic action, you have always the presence of septic products," that is of Bacteria. Now if Bacteria by their germs do not normally exist in the tissues of animals, and if you can determine their presence there at will under conditions which, as Dr. Sanderson says, "preclude the possibility of the introduction of any infecting matter from without," what must be the mode of origin of the Bacteria in such cases, and how can Dr. Sanderson do other than yield his assent to the doctrine of "Spontaneous Generation," or Heterogenesis, so far as the origin of Bacteria is concerned?

University College, April 6 H. CHARLTON BASTIAN

## Earthquake in St. Thomas

On the morning of the 11th instant at 4.30 A.M., a smart shock, accompanied by a rumbling noise, like that of a waggon rolling over rough pavement, travelling, as is usual here, from east to west, woke up the inhabitants of St. Thomas. It was followed within a few seconds by another shock, to the full as abrupt in its character as the first; the movement appeared to be not so much undulatory as vertical.

The concussion produced was felt still more distinctly within the harbour itself, where the jar communicated to the ships resembled, as one of the captains described it, that which might be produced by a heavy bale falling through the hatchways into the hold. Simultaneously the water of the bay, then perfectly still, assumed a turbid appearance, as though clouded by mud and sand; and a little later the surface was agitated by a strong ripple from the south, lasting some time.

On the same morning early the royal mail steamer *Corsica*, commanded by Capt. Herbert, was at anchor discharging cargo off the harbour of Dominique, about 170 miles distant from St. Thomas, S.E. The harbour is on the side of the island, and sheltered from the swell produced by the trade winds; the weather calm. Just about 5 A.M. a succession of heavy rollers broke in; they lasted for half an hour, and rendered all communication with the shore during that space impossible. No shock was felt on board the *Corsica*, but Captain Herbert caused note to be taken of the marine phenomenon, not doubting that it must have been due to an earthquake, as indeed was evidently the case.

The centre of disturbance would appear to have been in this case under the sea at some distance S.E. from St. Thomas, a direction often indicated in such occurrences. On one occasion only, that of the severe shock of November 1867, did the movement seem to have been propagated from due south, its centre

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